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ABSTRACT

This paper considers how educators can best arrive at "big, good ideas" for the improvement of higher education and how they can best put them to use where they are needed. Getting big, good ideas probably comes from working on big, good questions. There is a substantial body of research literature about the big questions of education, and it is evident that the "big" ideas come from "big scholarship" that is integrative, pragmatic, collaborative, and cumulative. Educational research needs additional funding, from more diverse sources, and researchers need to communicate what they know to practitioners and the public. Collaboration will enhance scholarship and that will lead to the big ideas that result in better learning. (SLD)

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Thinking Big: The State of Scholarship on Higher Education

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Higher education is entrusted with a sacred social responsibility: preserving, extending, and applying the fund of human intelligence and wisdom. No other institution does it; no other institution can do it. The study of higher education analyzes how colleges and universities do these things. Figuring how to do these things better is the test on which our field will succeed or fail. It will succeed if we answer big, important questions about how to do these things, and if we teach our students what we learn and share our knowledge with those who care about the institutions we know as colleges and universities. My task today is to think out loud about big questions and good answers and the usefulness of our work.

I did this once before from behind the scenes when I organized a panel for ASHE's first stand-alone meeting in San Antonio – in 1986. I asked George Keller, Yvonna Lincoln, and Marv Peterson to talk about the state of the field. Their assignment was to play off of George's famous "Trees without Fruit" article (Keller, 1985), a piece that said our research (and therefore our literature) was becoming so narrow, specialized, and inwardly focused that it risked having no impact on policy or practice. Of course, George said it more memorably than that, and more forcefully, too.

The panel never came to a clear conclusion, but that wasn't the point. I thought it was important to air the issue. I had recently completed four years as an assistant to the president of a major research university, and, although I had once been very excited about the application of political theory to the study of governance, I had found very little interest among my administrative colleagues in any kind of theory whatsoever. The calculus of administration and policy-making seemed to me to be very much about power and very little about ideas.

On the other hand, I could see clearly that power flowed at least in part to those whose ideas stood up in real life. Some ideas – some theories – were obviously better than others

because they gave us leverage over big problems. People who knew history had more power than others because they could reproduce earlier experience and keep the ambitious from making mistakes that others had made. People who knew economics had more power than others because they knew how to get the most out of scarce resources. People who knew human behavior had more power because they could bring people together in communities of purpose. People who knew literature had more power because they understood the trials of the human spirit and could say and do the right thing in hard times.

Big ideas and good ideas can and do make a difference. Perhaps I can best serve the occasion by asking you to think with me about how we get big, good ideas and how we can best put them to use where they are needed.

Big questions.

Getting big, good ideas probably comes from working on big, good questions. In a very short time, at least geologically speaking, we have learned to study our own institutions and to achieve something beyond the merely conventional wisdom that sufficed in the past. Higher education was highly "traditional" in the full sense of that word until at least the immediate post-World War II era. Our colleges and universities simply did what they had long done, and did it without much reflection or critical analysis. Modernity hit them as wave upon wave of new students with newly expanded individual rights and liberties and with the greater market muscle provided by student financial aid hit campuses in the period between 1950 and 1980. Self-study and thoughtful, reflective analytical work on "higher education" emerged and matured during this period. We became "modern."

This new "modernity" enabled us to think deeply about how students learn, grow, and develop and how academic work can be both productive and satisfying and how loosely coupled

organizations can be accountable and efficient. It borrowed from basic ideas developed in other fields, and showed how higher education could be a fertile ground for original research and creative conceptualization.

For all that we have accomplished, though, I am continually impressed with how little people know about higher education, how uninformed even nominal leaders of our institutions are about what we know. Because we can learn from their questions, and because we can help in their work I will argue on behalf of connecting with leaders and practitioners in the field. How we answer the big, important questions that come at us from those who fund us and set policy that governs us is a serious test for the maturity of our field and our own intellectual resilience and honesty.

These questions come up over and over again: What do students get out of college? Why does college cost so much? Why should faculty have tenure? (And why don't they have to work very much?) What good is research? Can't colleges be run like businesses? And so on and so forth. We know we could consult the work respectively of Pascarella and Terenzini, Sandy Astin, Ronald Ehrenberg, Henry Rosovsky, Donald Kennedy, Burton Clark, and others for perspectives. But why do newspaper reporters seem to write the same stories over and over again about part-time faculty who teach at three local colleges and can't land a full-time job no matter how well they perform. Or about the faculty member with a teaching award who failed to get tenure. Or about the family with three kids in college and who can't qualify for certain types of financial aid. It is almost as if reporters have a template for these stories that they carry with them and pass on to each succeeding reporter who covers higher education. More importantly, why do policy makers seem to try to solve the same problems with the same nostrums generation

after generation? Haven't we made ourselves clear about what we know, and know well? Haven't we connected...?

Do these questions have answers?

Serious students of higher education can answer these questions with deep perspectives and understanding. The maturation of our field has led to progressively deeper and broader and more sophisticated comprehension of both method and logic in confronting some of these eternal issues. Our efforts are more fruitful than ever because we have more and better data (for example, the NPSAS, NSOPF, and IPEDS data bases from the National Center for Education Statistics) with which to work. Some of our own colleagues (Lincoln, Creswell) have become leaders in expanding our repertoire of methods to the whole range of qualitative techniques.

The arts of synthesis, whether via meta-analytic techniques or via integrative literature reviews, have produced major landmark works of original scholarship on student development (Astin, Pascarella and Terenzini). The now-established Higher Education: Handbook of Theory and Research is a trove of integrative scholarship on a full range of issues and questions of both basic and contemporary questions.

Our major journals, The Journal of Higher Education, The Review of Higher Education, and Research in Higher Education continue to benefit from a stream of creative and thoughtful scholarship. This scholarship is emerging from a widening pool of talented people – often from fields outside our own – who have mastered a broad array of methods and ideas to shed new light on old questions.

I can think of no substantive question I might be asked about higher education on which I could not turn to a responsible research literature for perspective.

Specific substantive strengths of higher education scholarship.

The study of higher education has emerged in the past 50 years as an object of attention and interest by scholars from a variety of traditions. To mention just a few examples, the following come to mind. Historians (Rudolph, Metzger, Thelin), economists (Howard Bowen, William Bowen, Breneman, Johnstone), political scientists (Berdahl), psychologists (Astin, Chickering, Patricia King, Claude Steele, Pascarella, and Terenzini), sociologists/anthropologists (Clark, Dorothy Finnegan, Sylvia Hurtado, Gary Rhodes), organizational theorists (Birnbaum, Cameron, March), legal scholars, (Barbara Lee, William Kaplin), postmodernists (Slaughter, Tierney), and policy analysts (Hearn, Zumeta) have all turned their scrutiny to a wide array of topics and issues at the foundations of how higher education is studied and "practiced." In a different way, higher education institutions and their assorted participants have provided data for the analysis and advancement of theory in a variety of disciplinary arenas – the work of Ronald Ehrenberg and Gordon Winston on the economics of higher education comes to mind quickly. Burton Clark has (like David Riesman and Christopher Jencks) enriched sociological perspectives with his analysis of higher education. Cohen and March and Karl Weick have used their own observations of higher education to bring creative insights to organization theory. Many postmodernists rely on their experience in higher education to generalize and theorize about culture.

Scholars trained in or committed to the study of higher education have made their own original contributions to progress on pressing issues of public concern. Some (Stanley Ikenberry, Kenneth Mortimer, David Gardner) have done so as presidents of prominent public institutions. Some have held major policy-making positions (David Longanecker). Others have done so with their scholarly work (Kenneth Tollett, Sheila Slaughter and Larry Leslie, Ed St. John, Jacqueline

King, Mike Nettles). And influential voices regarding race and ethnicity in American culture have spoken from their perspectives on higher education (William Bowen and Derek Bok, Johnetta Cole, William Gray, William Harvey, Freeman Hrabowski, Yolanda Moses, Michael Olivas, Laura Rendon, and Donald Stewart, among our contemporaries).

In short, we have gotten far beyond what social scientists-turned-administrators like Lester Anderson and T. R. McConnell began with the establishment of "centers for the study of higher education" that drew young scholars into academic careers in this emerging field.

It isn't possible to recapitulate all the lines of research that now serve as foundations for both policy and practice in higher education. But we know (and present to the concerned public) more than ever before thanks to the cumulative efforts of serious and dedicated scholars. I think the lesson we can take from those who have made an impact is that they stay focused on big ideas, ideas that help us get leverage and understand things in useful ways.

Getting big ideas.

I'd like to take this notion of "big ideas" as a starting point in thinking about how our field can perform more strategically. Without elaborating in detail, let me just stipulate that big ideas come from "big scholarship." Big scholarship is integrative, pragmatic, collaborative, and cumulative. (It is ironically also competitive and individualistic, but I will ignore the ironies here.)

Big scholarship is "big" precisely because it has impact on how society sees and manages its sense of reality. It is big because it works. It works because it brings results. Big scholarship is the offspring of the Scottish Enlightenment (Herman, 2001). As I understand the term, it refers to the predisposition of 18th century Scottish intellectuals to put empirical observation and practical experience at the foundation of all inquiry. Engineering, medicine, economics, geology,

and even contemporary methods of instruction in American higher education owe this frame of mind a great debt. Herman contends that "the Scots invented the modern world" with their spirited pragmatism and their rejection of tradition, myth, and privilege.

How can we benefit from the qualities of big scholarship and how can we use them to re-center our work on things that count most? I don't see our work as deficient in these qualities, but it never hurts to keep them in clear focus.

Integrative. The higher education literature, like literature in other fields of study, betrays a basic tension between the theoretical and the empirical. It happens in physics where the theoreticians and the experimentalists duke it out routinely. And in economics, and medicine, and even in English, where those who argue that text is all and those who argue that perspective is all perennially see things differently. In this regard, we are mainstream. In point of fact, observation and theorizing are just two parts of the great "wheel of science" (Light, Singer and Willett) that describes inquiry as a continuing cycle of logic: inductive reasoning from evidence to theory, and deductive reasoning to test propositions against empirical reality. But we do know that without empirical evidence, theory risks lunacy. Coventry Patmore, the English poet, put it this way. "Nearly all our disasters come from a few fools having the 'courage of their convictions.'" On the other hand, raw data is simply a pile of facts without meaning until serious efforts are made to draw inferences and meanings from it. Inferences and meanings, of course, are part of the perspectives we carry around with us, and, when systematized, amount to "theory," or dependable explanations.

So you can't have one without the other. Theory is worthless without observation; observation is worthless without theory. One of the problems a maturing or mature discipline must resolve is the tension between the two. But the ineffability of "data" is a fundamental

problem in the study of humanity and culture and institutions. What do you observe? Subjectivity and objectivity and constructivism and deconstructivism all converge. This is another tension that needs to be worked out. Philosophically, we have both ontological and epistemological issues that simply can't be waved away: What do we know and how do we know it?

The big important problems demand an integration of theory and evidence (or vice versa). Understanding how students learn; understanding how to construct campus environments and experiences that enhance learning, growth and development; understanding the economics (and efficiencies) of colleges and universities; understanding how to organize people in a culture that values freedom and autonomy – all bear directly on how effective higher education is, and how well it serves its students and society. Big scholarship explains, a serious intellectual giant step beyond merely illuminating, commenting, or connecting pieces of evidence to each other. Integrating what we observe with what we understand and modifying both evidence and perspective are key to big scholarship.

Pragmatic. Big scholarship is pragmatic. It has payoff in many ways. While theory may be an end in itself, it is arguably better when it is useful, too. And obviously, the payoff reaps payback. Science that is useful is science that attracts funding. But morally speaking, it seems to me that one obligation of scholarship is to do good. Application is a product of thinking beyond big ideas to their application. In other words, a field like our's is a ripe ground for translation of theory to practice.

The trap in this game, though, may be translating practice to practice. A field can turn so pragmatic that it becomes no more than an intellectually empty bazaar at which hawkers trade their own experiences and successes and failures. This "war story" culture is a problem in any

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applied field. Engineers learn from their last mistake, physicians from their last patient, educators from their last students. But if all they do is learn from the last one, there is really nothing lasting about it. Once again, the value of theory becomes apparent.

Praxis – accumulated experience and the resulting knowledge – is soundest when it is buttressed with (and buttresses) theory. We may feel that discussion works better with some students and lecture works better with others. We may even share these insights among ourselves and make many decisions about teaching that draw on these insights. But it would pay to put a little science up against folk wisdom to turn it into real "praxis." Knowing "what works" in William Bennett's memorable language is a long way from knowing why it works. This is where Bennett went badly wrong – cutting to the chase while cutting scientific corners. Imagine the long-term payoff if we could actually validate praxis with theory – understanding why certain practices work or don't work. More experimentation or quasi-experimentation, systematically observed and analyzed, would be healthy for our own field and many other applied fields.

Cumulative. That leads me directly to another property of big scholarship: cumulativity. The monumental work of Ernie Pascarella and Pat Terenzini exemplifies what I mean. Integrative scholarship that pulls results together across time is what my mother used to call "learning from experience." She always said it was a good thing and always rationalized mistakes and failures by saying this was good because we'd learn from our experience.

Each generation assumes it is discovering grand new principles, but Rich Novak and I recently published a paper in which we examined the ambient policy questions from the 1930's about governing and financing higher education. The questions and answers were remarkably parallel to questions and answers about the same issues, circa 2000 (Novak and Leslie, 2001). Another example is my sabbatical leave project to learn about the ideas and impact of Nikolai

Grundtvig on Danish education in the 1830's and 1840's (Leslie, 1994). By the time I wrapped my study up, I had concluded that John Dewey learned everything he knew from Grundtvig – but I can find no references to Grundtvig anywhere in the Deweyan canon. Is the "constructivism" of 2002 really so much different? I hear echoes of both Grundtvig and Dewey in the talk and papers of my constructivist colleagues.

Reinventing the wheel is probably inevitable to some extent, but it seems to me that cumulativity in our scholarship might help us to solidify what we know or don't know and help us move on to higher level scholarship that integrates, confirms, and assesses the body of knowledge.

Those who don't learn from the past are condemned to repeat it, said Santayana. Theory building is basically about learning across the generations of human experience, accumulating tested wisdom so we don't have to keep rediscovering things for ourselves. Learning from experience, or building theory, saves a lot of time and effort and heartache, and accumulating tested wisdom is what universities are about at their very core.

Collaborative. Big scholarship involves highly collaborative work among colleagues world-wide. In some cases, this may mean actually working together. It might also mean competing against one another – in the most responsible ways.

Competition in the contest of ideas is important and moves the formation and testing of theory ahead. I'd like to see a lot more of this competitive collaboration instead of superficial agreement on hot ideas. Take the notion of "transformational leadership," a hot idea that seems thankfully to have had its day. I was struck over a period of years by how the academic and popular literature perverted Burns' sophisticated parsing of a necessary balance between transactional and transformational leadership. Popularizers chose to ignore the transactional part

and spread the "transformational" gospel. I have yet to see real tests of Burns' formulation among researchers who would test the main propositions in varied settings to see what holds up and what doesn't. I like to imagine a wide network of competitive collaborators gathering data – cumulatively, of course – and meeting periodically to debate their substantive findings. They might ultimately achieve a powerful working synthesis of leadership theory.

Instead, one is left with the absurdly unbalanced conception of leadership as a mostly transformational exercise. It is preposterous. If all leaders do is transform organizations, then everything is always in a state of transformation, and evidence and experience say that is patently not true. Forcing scholarship into more tightly collaborative efforts, I think, would override intellectually immature and irresponsible popularization.

What next....?

Before I turn to the qualities I'd like to see in our scholarship, let me digress to mention funding, something that has constrained our field in serious ways. I mean no offense to those whose good efforts have characterized our federally funded research centers over the last 20 years, but putting so many funding eggs in one basket has left our field gasping for support. The small amount of grant funding that has gone into research on higher education, one of the most profoundly important social institutions of the past two centuries, astonishes and befuddles the thinking mind. Politicians will rave about the importance of college, but they will also deny (and even denigrate) the importance of scholarship, and have certainly done little or nothing to support research or training to assure the flow of new understandings and real improvement.

Obviously, a better and more reliable model for funding research on higher education is badly needed. I suggest three criteria that should underlie that model: 1) Funding agencies should cooperate on establishing priorities. We now have a crazy quilt of foundations and government

agencies that produce a random agenda of projects. 2) Funded research should be integrative and cumulative. While there is sometimes glamorous payoff in venture capital for research on exciting new ideas, the greater payoff is in building on solid foundations. Funding priority should go to projects that respect cumulativity and integration of knowledge. 3) Funding should develop new talent. Younger researchers need support, and jump-starting their agendas can have a long-term effect on the coherence and systematization of the field over generational period. Competitive awards are the life-blood of a vital scholarly enterprise, but carving out some space for jump-starting the plans and programs of promising young scholars strikes me as a sound way to invest in the future of our field.

The study of higher education has reached an interesting point in its development. Joe Beckham and I wrote an essay in the Review of Higher Education drawing on that 1986 ASHE panel (Leslie and Beckham, 1986). We thought the most important thing was for our research community to avoid solipsism – a sorry state in which we mainly talked to ourselves.about ourselves. Rather, we thought – and I still think – it is important to apply the stimulus of practical engagement to the questions we think about and study.

I know how important it is for scholars to develop their own conversations, and to work on each other's logic and methods. Without that kind of competitive collaboration, we really do risk solipsism. But how much richer our conversation would be if we invited more of our practitioner colleagues to join us and submitted our thoughts to their questions. Big ideas need big questions. Wouldn't it be interesting if some of the critics at each of our session were practitioners – presidents or deans or legislative staff, for example.

But the converse is true, too. Big questions need big ideas. So we have to find the language and the media through which to communicate what we know to people who ask

challenging questions. How often do we present at meetings outside our little world here at ASHE? Should we be testing our ideas against those of scholars in other disciplines? Would the challenge of talking to these groups lead us to reframe our ideas and our ways of talking about them? What does it take to communicate big ideas to audiences of administrators, legislative committees, and journalists?

I have tried many times to bring a practitioner colleague – three former presidents, and some One Dupont Circle staffers in recent memory – to co-author and co-present at ASHE or AERA meetings. It is always a challenge to educate them to the folkways of scholarship, and it is always a challenge to integrate their points of view in a way that blends both theory and practice. It is not always comfortable or successful, but I always come away thinking it was worth trying.

The main thing is that we need ways to keep big questions in front of us. Those questions come in very interesting and challenging ways from "away," not from within the family. The more we tackle big questions, the more likely we'll come away with big ideas and the more likely those big ideas will take root in better ways to educate. In a nutshell, that is my sense of our biggest challenge. As E. M. Forster wrote in Howard's End, "Only connect! .... Only connect the prose and the passion, and both will be exalted, ... Live in fragments no longer. Only connect, and the beast and the monk, robbed of the isolation that is life to either, will die." Only connect....connect our scholarship to problems that pose big questions. Only connect....with as many different points of view as you can find, especially those from "away." Only connect.... with the vital problems of practice and operation that colleges and universities face every day. Only connect ..... to past scholarship, assessing it critically and learning from experience. Only connect....through competitive collaboration with our colleagues. Test them and be tested in return. Only connect .....our research to the mission of higher education, which

is, as it has always been, to preserve, extend, and apply human wisdom to big and timeless problems of life. The goal of our field is to understand how to do that in institutions that exemplify and advance human creativity.

Let me conclude by reemphasizing two themes:

"Only connect...." The idea is to forge links between observation and theory. To forge links between research and practice. To forge links between past and present. And to forge links across intellectual and cultural boundaries.

"Think big...." Big scholarship leads to big ideas. Big ideas lead to greater good. Greater good in our field means better colleges and universities. We will ultimately answer for the impact of our ideas on colleges and universities, on those who come to them to learn, and on those who depend on them for results. The marketplace of ideas we have created with ASHE is the best and freest place in which to think big. And I hope all of you will dedicate your careers and your scholarly work to thinking big – and by doing so, that you will take responsibility for making higher education better. Better in ways that we can understand and explain to politicians and presidents, to the public and practitioners, as well as to one another.

Only connect. Think big. Arise, go forth, conquer.

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